THE IMPACT OF MEDICAID EXPANSIONS ON MORTALITY*

Benjamin D. Sommers
Department of Health Policy & Management
Harvard School of Public Health
bsommers@hsph.harvard.edu

Abstract

Previous research found that Medicaid expansions in New York, Arizona, and Maine in the early 2000's reduced mortality. I revisit this question with improved data and methods, exploring distinct causes of death and presenting a cost-benefit analysis. Differences-in-differences analysis using a propensity-score control group shows that all-cause mortality declined by 6%, with the most robust reductions for healthcare-amenable causes. HIV-related mortality (affected by the recent introduction of antiretrovirals) accounted for 20% of the effect. Mortality changes were closely linked to county-level coverage gains, with one life saved annually for every 239-316 adults gaining insurance. The results imply a cost-per-life saved ranging from $327,000 to $867,000, which compares favorably to most estimates of the value of a statistical life.

Keywords: Medicaid, health insurance, mortality
JEL Codes: I13, I18, I38, D31
Version Date: December 22, 2014

* DISCLOSURE / ACKNOWLEDGMENTS: This work was supported in part by grant number K02HS021291 from the Agency for Healthcare Research and Quality (AHRQ). I am grateful to Kate Baicker, Arnie Epstein, Joe Newhouse, and Kosali Simon for thoughtful comments on earlier drafts of this manuscript, and to the participants in the health seminar at Yale's Institution for Social and Policy Studies for helpful suggestions on related work. The author currently serves part-time as an advisor in the Office of the Assistant Secretary for Planning and Evaluation, at the U.S. Department of Health and Human Services (HHS). This paper does not represent the views of HHS or AHRQ.

I. INTRODUCTION

The major expansion of Medicaid eligibility under the Affordable Care Act (ACA) – and the subsequent flexibility granted to states by the Supreme Court to opt out – has prompted a renewed debate about the value of Medicaid coverage. While evidence from the Oregon Health Insurance Experiment (OHIE) and other recent studies have demonstrated the program's value in reducing financial risk to beneficiaries and increasing access to health care services (Baicker et al., 2013; DeLeire, Dague, Leininger, Voskuil, & Friedsam, 2013; Finkelstein et al., 2012; Sommers & Oellerich, 2013), the literature is more ambiguous about the impact of Medicaid on health. This, is turn, has fueled significant debate about the relative economic merits of spending billions of additional public dollars on the program.

In the landmark OHIE, which studied the effects of a lottery that randomly offered approximately 10,000 low-income adults the opportunity to enroll in Oregon's Medicaid program, acquiring Medicaid led to significant improvements in self-reported health and mental health, but no statistically significant changes in several physiologic measures. Meanwhile, in a much larger but non-randomized analysis of three state Medicaid expansions in the early 2000s, Sommers, Baicker, and Epstein (2012) used a differences-in-differences framework and found increases in self-reported health and a significant decline in population-level mortality over a 5-year follow period for non-elderly adults, compared to adults in four neighboring states without Medicaid expansions. The OHIE results – along with several specific concerns about the methodology of the Sommers et al. paper – have left some analysts to conclude that Medicaid offers no health benefits to beneficiaries (Roy, 2013).

Here, I revisit the Medicaid expansions in three states – New York, Arizona, and Maine – which extended Medicaid eligibility to a similar population of low-income adults as the OHIE and

ACA expansions,[1] with several important methodological improvements. Using restricted access microdata (1997-2007) from the Centers for Disease Control and Prevention (CDC), which captures detailed information on every death occurring the United States each year, I develop a more ideal control group for these expansion states, matching counties in those states based on the pre-expansion mortality trend, as well as county-level demographic and economic indicators. I also use these more detailed data to explore specific causes of death, including healthcare-amenable causes of death – which previous research suggests may be more responsive to better access to medical care (Nolte & McKee, 2003; Sommers, Long, & Baicker, 2014) – and deaths related to HIV, since the introduction of highly-aggressive antiretroviral therapy for HIV in the late 1990s led to well-documented declines in mortality that coincide with the identification strategy. Finally, I link these mortality data to county-level changes in the uninsured rate from before and after the Medicaid expansion, to test for the marginal impact of the coverage expansion and conduct a cost-benefit analysis of the mortality changes.

The primary findings are threefold. First, using more detailed data and a stronger identification strategy, I find robust evidence of an impact of Medicaid expansions on all-cause mortality similar to previous research. After Medicaid expansions in these three states, the population death rate for 20-64 year old adults declined by 6%, or roughly 20 deaths per 100,000, when compared to adults living in demographically and economically similar counties in non-expanding states. Placebo and linear trend testing showed no evidence of divergence between the control group and expansion states prior to 2001.

[1] New York expanded eligibility to childless adults with incomes up to 100% of the federal poverty level and parents with incomes up to 150% of the federal poverty level in September 2001. Arizona expanded eligibility to childless adults with incomes below 100% of the federal poverty level in November 2001 and to parents with incomes up to 200% of the federal poverty level in October 2002. Maine expanded eligibility to childless adults with incomes up to 100% of the federal poverty level in October 2002.

Second, mortality changes varied by cause of death. Declines in mortality were most robust for deaths from so-called health care amenable causes, which declined by 6.7%, while deaths from other causes showed only a marginally significant decline. While results for New York (the largest state with the largest proportional expansion) drove the all-cause mortality declines, analyses excluding New York suggest that health care amenable mortality may have declined after expansions in Maine and Arizona as well. Meanwhile, HIV-related mortality was declining quite rapidly during this period, and HIV was much more prevalent at baseline in expansion states than in the control group. HIV-related mortality declines accounted for 20% of the overall mortality effect in this study. However, the relative decline in HIV-related mortality was nearly twice as large in Medicaid-expanding states as in non-expanding states, suggesting that expanded insurance coverage worked in tandem with newly-available antiretrovirals to produce larger health impacts.

Third, mortality changes were closely linked to county-level changes in insurance coverage, supporting the conclusion that expansion of health insurance rather than other contemporaneous changes were driving the changing death rates. A difference-in-difference-in-difference (DDD) model using elderly adults produced similar findings, further supporting the expansion of insurance coverage to working age adults as the most likely causal mechanism. Point estimates suggest that for every 239 to 316 adults gaining health insurance, one death was prevented each year. Using data from the OHIE of increased per-person health care spending from acquiring Medicaid, this suggests a societal cost of $327,000 to $867,000 per life saved (in 2007 dollars), depending on assumptions about the deadweight loss of public financing.

The paper is organized as follows. Section II reviews the literature on the relationship between mortality and health insurance in general, and the impact of Medicaid in particular.

Section III describes the data and methods. Section IV presents the results for all-cause mortality and analyses by cause of death. Section V presents two additional specifications – a DDD model with elderly adults, and a model linking changes in mortality with county-level reductions in the uninsured rate – and a cost-benefit analysis of the mortality changes. Section VI concludes.

II. PREVIOUS LITERATURE

There is a lack of consensus in the literature about the impact of health insurance on mortality. Studies of Medicare among the elderly have found mixed results, with some finding a reduction in in-hospital deaths associated with the creation of Medicare in 1965 (Card, Dobkin, & Maestas, 2009), while a broader population-level analysis did not find any mortality changes among adults 65 and older during the same period (Finkelstein & McKnight, 2008). Longitudinal analyses of private insurance have found large unadjusted differences in survival between insured and uninsured adults, but studies have differed as to whether adjustment for underlying health differences fully explain the survival gap (Black, Espin-Sanchez, French, & Litvak, 2013; Kronick, 2009) or still leave a residual mortality benefit of insurance (Wilper et al., 2009). Most recently, a quasi-experimental analysis of Massachusetts' 2006 health reform found a significant reduction in mortality for working-age adults compared to a propensity-score matched set of control counties in other states, with mortality changes concentrated among healthcare-amenable causes of death and in counties with higher baseline uninsured rates (Sommers, Long, et al., 2014).

With regard to Medicaid more specifically, a large body of research demonstrates that Medicaid coverage leads to significant gains in access to care and financial risk protection, compared to being uninsured. These studies include cross-sectional analyses using state eligibility as an instrument for Medicaid coverage (Long, Coughlin, & King, 2005), differences-in-differences analyses of state and federal expansions of Medicaid eligibility (Currie & Gruber, 1996a, 1996b; Sommers, Baicker, & Epstein, 2012), and the recent randomized trial in Oregon (Finkelstein et al., 2012).

However, evidence on Medicaid's impact on health and mortality has been mixed. Currie & Gruber (1996a, 1996b) showed that expansions of Medicaid eligibility to pregnant women and children in the 1980s led to reductions in infant mortality and children's mortality, though other analyses (Epstein & Newhouse, 1998; Howell, 2001) found little or no effect. In a recent working paper, the initial implementation of Medicaid in the late 1960's was linked to significant reductions in infant and child mortality for non-whites, with an estimated 40% individual-level reduction in death rates (Goodman-Bacon, 2013).

Meanwhile, among adults a potentially more definitive study exists due to Oregon's fortuitous use of a lottery to randomly select low-income adults from a waitlist for the opportunity to sign up for Medicaid. In several landmark papers, the OHIE showed that gaining Medicaid led to strong and nearly immediate improvements in self-reported health, major reductions in depression, and increased use of recommended services such as cancer screening and medication for diabetes; but notably, over a mean follow up period of 18 months, there were no statistically significant changes in blood pressure, cholesterol, or glycated hemoglobin (Baicker et al., 2013; Finkelstein et al., 2012). While OHIE did examine mortality at one year, with no significant changes detected, the confidence interval was extremely wide and could not rule out sizable mortality reductions (or increases).[2]

[2] Finkelstein et al. (2012) used an instrumental variables analysis of the lottery for Medicaid eligibility to identify the local average treatment effect (LATE) of acquiring Medicaid. They measured a 0.0013 percentage point decrease in the likelihood of death compared to the control group, with a 95% confidence interval of [-.0066, .0040], from a baseline mortality rate of .008. Thus, their estimates imply a confidence interval for an individual-level mortality change of -82% to +50%.

In the absence of any randomized trial of adequate size to settle this question, the largest quasi-experimental analysis to date of Medicaid's impact on adult mortality comes from Sommers, Baicker, and Epstein (2012). Using population-level vital statistics and additional survey-reported outcomes, the researchers showed that large state Medicaid expansions in 2001-2002 in New York, Arizona, and Maine led to a 3.2 percentage point decrease in the uninsured rate, gains in access to care and self-reported health, and a mortality decline of roughly 20 deaths per 100,000 working-age adults, relative to adults in neighboring states without any expansions. Mortality declines were concentrated in lower-income counties and among non-whites.

However, there are several potential threats to the identification strategy in Sommers et al. (2012). First, concerns about differences in demographics and pre-expansion trends between treatment and control states have been raised (Kaestner, 2012), and no direct attempt was made to select a control group based on pre-expansion mortality trends. Second, the results were largely driven by New York, by far the most populous state in the study, and New York has several unique features that distinguish it from other expansion and control states in the original sample. For instance, New York experienced a major increase in mortality in 2001 due to the terrorist attacks of September 11th, which may have affected tests of pre-expansion parallel trends. Third, the Medicaid expansions – which were implemented in 2001-2002 – were concurrent with the ongoing diffusion of highly-effective treatment for HIV in the form of antiretroviral medications, which led to a dramatic decline in HIV-related mortality nationwide beginning in 1996 (Duggan & Evans, 2008; Palella et al., 2006).[3] HIV deaths at baseline were significantly higher in New York than in any other state in the study, raising the possibility that new medical technology rather than insurance coverage may have driven some of the observed mortality decline.

[3] National age-adjusted HIV-related mortality for non-elderly adults peaked at 23.8 per 100,000 in 1995 and had declined to 8.8 by 1997 and 5.3 by 2007, according to CDC official statistics (www. wonder.cdc.gov).

Finally, the authors' county-level mortality analysis was subject to the ecological fallacy and did not include any information on insurance coverage at the individual or even county level. While analogous differences-in-differences analyses of Census data enabled the authors to identify a significant reduction in the uninsured rate in these expansion states (with essentially no private insurance crowd-out), they were unable to directly tie this finding to their mortality data.

Here, I explore the impact of these three states' large Medicaid expansions, with an improved data source and special attention to the most critical threats to validity from the original analysis: the comparability of pre-expansion trends and control state demographics, the potential biases of September 11th and HIV antiretroviral therapy, and the lack of a direct link between insurance coverage and the observed mortality reductions.

III. DATA & METHODS

III.A. Data

The primary data come from national vital statistics collected by the Centers for Disease Control and Prevention (CDC). The analysis by Sommers et al. (2012) used the publicly available Compressed Mortality File, which reports annual mortality rates by county age-race-sex cells. However, in the public use file, the CDC suppressed all cells with death counts between 1-5, and Sommers et al. had to impute these suppressed values for 5% of the weighted sample. These suppression rules and imputation may have introduced bias and precluded more detailed analyses by causes of death.

To address these limitations, instead of the public use Compressed Mortality File, I obtained access to the complete U.S. Vital Statistics dataset. This dataset is available only via direct application to the CDC,[4] and contains the following information on every U.S. death occurring each year: age group, sex, race, cause of death, and county of residence.[5] The CDC pairs this information with the corresponding population denominator by county, age, sex, and race (from Census data) to allow for the calculation of an annual death rate. Causes of death are based on the International Classification of Diseases Versions 9 (1997-1998) and 10 (1999-2007).

These data were then merged with several county-year specific economic indicators – the unemployment rate, median income, and poverty rate – from the Area Resource File, since differential changes in the economy in expansion vs. control states may threaten the identification strategy.[6] In addition, I obtained county-level measures of the uninsured rate among the 19-64 year-old age group from the Census Bureau's Small Area Health Insurance Estimates.

III.B. Methods and Control Group

The primary study design was a differences-in-differences analysis comparing annual mortality in county race-sex-age group cells in the three expansion states – New York, Arizona, and Maine – to a control group of counties without Medicaid expansions. I improve on the control group used in Sommers et al. (2012) by matching to the subset of U.S. counties that most resembled the counties in the expansion states based on the pre-expansion mortality trend, baseline demographics, and economic conditions. The study period is 1997-2007, with the pre-expansion

[4] CDC data use policies prohibit the author from directly sharing this dataset, but the application for data access is available at http://www.naphsis.org/Pages/VitalStatisticsDataResearchRequestProcess.aspx
[5] Location of death is based on the individual's residence. If a person dies in a county other than where they live, his/her death is counted in mortality statistics for their home county.
[6] Interestingly, it is unclear what direction the potential bias would be from differential economic changes. While numerous studies document a major detrimental effect of lower socioeconomic status on survival, others have found that mortality decreases during recessions (Ruhm, 2005, 2009). In any event, adjustment for these economic indicators does not have a major impact either way on estimates of the mortality changes due to Medicaid expansion in this paper.

baseline period defined as 1997-2000, and 2001 omitted from the sample entirely as both a transitional year for the expansions and to avoid any bias from the attacks of September 11th.[7] The approach of using a propensity-score model to identify an appropriate control group of counties is similar to that in Sommers et al.'s 2014 analysis of Massachusetts' health reform. This approach resembles the synthetic control group method (Abadie, Diamond, & Hainmueller, 2012) in that it matches on pre-intervention trends in the outcome, but additionally incorporates information on key covariates that are important predictors of the outcome and that differ significantly across counties. The tradeoff of adding additional elements for matching is that the precision of the match for any one dimension may decrease as other factors are used in the propensity match. Thus, pre-expansion trend testing and placebo testing are still critical for assessing the identifying assumption.

The propensity score model was fit using the following county-level logistic regression for the whole U.S. in the pre-expansion period, weighted by each county's population size:

$$(1)\ ExpansionCounty_c = \beta_0 + \beta_1\, UnemploymentRate_c + \beta_2\, PovertyRate_c$$
$$+ \beta_3\, MedianIncome_c + \beta_4\, \mathbf{X_c} + \beta_5\, MortalityRate97_c$$
$$+ \beta_6\, MortalityRate98_c + \beta_7\, MortalityRate99_c + \beta_8\, MortalityRate00_c$$
$$+ \varepsilon_c$$

where c indexes the county. *ExpansionCounty* was a dummy variable equal to 1 for counties in New York, Arizona, and Maine, and 0 otherwise. $\mathbf{X_c}$ is a vector of demographics (percentages of the county's population in each age group, race/ethnicity, and sex), and all economic and

[7] The treatment of the time frame differs slightly here from that in Sommers et al. (2012). In that paper, the time frame was limited to 5 years before and after each state's expansion, which means that the study period was 1997-2006 for New York and Arizona, and 1998-2007 for Maine. Here, I use the full overlapping time period to be consistent across all states (1997-2007), with year fixed effects to capture any national mortality trends in each year. County-level measures of the poverty rate and median income are not available in the ARF prior to 1997, precluding adding additional pre-expansion years to the sample without dropping these key covariates. Following the original analysis, I treat the first full year of coverage after each expansion as the first post-expansion year.

demographic variables were averaged over the 1997-2000 pre-expansion period. Demographics were based on the population of adults ages 20-64.[8] Coefficients β_5 through β_8 captured the annual mortality pattern for this same period. Then the model was used to generate predicted values for each county, indicating its similarity to the expansion counties' overall population and mortality pattern over time. The control group was then defined as the first quartile of counties (weighted by population size) most closely resembling the expansion states. This approach resulted in control counties being identified in all remaining 47 states but not the District of Columbia, thus yielding a final sample containing 50 state clusters.

To determine whether this approach yielded an appropriate control group based on pre-expansion mortality trends, I first tested for differential linear trends for the years 1997-2000 in the control states versus the expansion states. I then conducted a falsification test for any divergence in mortality trends in 2000 (the last full year before the first expansions began), using only the 1996-2000 pre-expansion data and treating 2000 as a placebo expansion year. The results of these analyses, reported in Section IV, support the identifying assumption of similar pre-expansion mortality trends. Equation 2 describes the linear trend testing, and Equation 3 describes the placebo test, both of which were analyzed using the pre-expansion data (1997-2000) and weighted by population size:

$$(2)\qquad \mathit{MortalityRate}_{ijkct} = \beta_0 + \beta_1 \mathbf{X_{ijk}} + \beta_2 \textbf{County-Level Factors}_{\mathbf{ct}} + \beta_3\, \mathit{Time\ Trend}_t + \beta_4\, \mathit{Expansion\ State}_s{*}\mathit{Time\ Trend}_t + \Omega\ \mathit{County}_c + \varepsilon_{ijkct}$$

where i indexed age, j race, k sex, c county, s state, and t year. $\mathbf{X_{ijk}}$ was a vector of demographics (age group, race, and sex). **County-Level Factors** included county-year-specific poverty rate,

[8] 19 year olds are classified in the mortality data as part of the 15-19 year-old age group, which is why the sample for working age adults for this analysis begins at age 20.

median income (in 2007 inflation-adjusted dollars), unemployment rate, and percentage of the population that is Latino.[9] Ω is a vector of county fixed effects. β_3 is a linear time trend, and β_4 measures any differential time trend for the expansion states, compared to the control group.

(3) $$MortalityRate_{ijkct} = \beta_0 + \beta_1 \mathbf{X_{ijk}} + \beta_2 \textbf{County-Level Factors}_{\mathbf{ct}} + \beta_3\, ExpansionState_s * Yr2000_{st} + \mu\, Year_t + \Omega\, County_c + \varepsilon_{ijkct}$$

Equation 3, which represents the placebo test for a differential mortality effect in the last pre-expansion year, replaces the linear time trend with μ, a vector of year fixed effects. Then β_3 captures the effect of living in an expansion state in the year 2000, the last year before any of the state Medicaid expansions. The regression used Huber-White robust standard errors clustered at the state level (n=50).

III.C. Analyses of All-Cause Mortality

The primary outcome is all-cause mortality, expressed as deaths per 100,000 adults. The unit of analysis is the year-specific age-sex-race cell within each county. Equation 4 shows the primary regression model:

(4) $$MortalityRate_{ijkct} = \beta_0 + \beta_1 \mathbf{X_{ijk}} + \beta_2 \textbf{County-Level Factors}_{\mathbf{ct}} + \beta_3\, ExpansionState_s * Post\text{-}Expansion_t + \mu\, Year_t + \Omega\, County_c + \varepsilon_{ijkct}$$

As above, *i* indexed age, *j* race, *k* sex, *c* county, *s* state, and *t* year. $\mathbf{X_{ijk}}$ was a vector of demographics (age group, race, and sex). **County-Level Factors** included county-year-specific poverty rate, median income (in 2007 dollars), unemployment rate, and percentage of the

[9] The CDC did not include ethnicity in the mortality data until 1999, and some death certificates after 1999 were still missing this information. I use the year-specific county-wide percentage of the population that is Latino, from the Area Resource File, to allow for the full sample to be included in the analysis.

population that is Latino. β_3 is the coefficient of interest, capturing the effect of being in an expansion state after Medicaid expansion. The direct effects of the post-expansion period and being in an expansion state are respectively captured by μ, a vector of year fixed effects, and Ω, a vector of county fixed effects. Robust standard errors were clustered at the state level (n=50) (Bertrand, Duflo, & Mullainathan, 2004).

The primary regressions use linear models, but I also tested the sensitivity of the results to an alternative functional form, using a generalized linear model (GLM) with a negative binomial distribution and log-link. This model is an appealing match for the mortality dataset, which features count data with overdispersion, and was used in a similar analysis in Sommers et al. (2014) to analyze Massachusetts' health reform. This model takes the same form as the linear model, except the dependent variable is coded as number of deaths, with population size as an exposure variable. The other difference is that the GLM model replaces the vector of county fixed effects with a vector of state fixed effects. The reason for this is that the GLM negative binomial model does not perform well when including numerous fixed effects (and often simply does not converge in the STATA estimation).[10] This model reports relative change in mortality, as opposed to absolute changes in the linear model.

III.D. Analyses by Causes of Death

I conducted two additional sets of analyses, analogous to the model in Equation 4, but with different disease-specific mortality rates. First, following previous work used in international comparisons of mortality (Nolte & McKee, 2008; Nolte & McKee, 2003) and in the analysis of Massachusetts' health reform (Sommers, Long, et al., 2014), I identify those deaths due to so-

[10] See additional discussion of this issue in Greene (2005), "Functional form and heterogeneity in models for count data," *Foundations and Trends in Econometrics* 1: 113–218.

called "healthcare-amenable" causes. These causes include cardiovascular disease, infections, cancer, diabetes, kidney disease, and other conditions thought to be more responsive to timely medical care.[11] Major causes excluded from this definition include accidental deaths, suicides, and homicides. There has been suggestive evidence that insurance status can reduce mortality even from some of the latter conditions – for instance, in-hospital mortality may be as much as 40% higher for trauma victims without health insurance compared to insured patients (Doyle, 2005), and the OHIE showed that the acquisition of Medicaid dramatically reduces depression rates (Baicker et al., 2013), the most common diagnostic risk factor for suicide (Brown, Beck, Steer, & Grisham, 2000). Nonetheless, it is reasonable to assume that any mortality effects mediated by insurance coverage would be larger for healthcare-amenable causes.

A second set of analyses examined deaths due to HIV and AIDS, and conversely analyzed mortality from all causes *other* than HIV. As discussed earlier, the introduction of highly aggressive anti-retroviral therapy (HAART) in the late 1990s and early 2000s led to major reductions in HIV-related mortality (Palella et al., 2006), and this trend was concurrent with these Medicaid expansions. Separating out this cause of death and looking at the relative declines in mortality between expansion and control states can shed light on the extent to which the all-cause mortality changes are attributable to HIV alone, and also whether Medicaid expansion facilitated the mortality reductions of HAART among those with HIV.

IV. RESULTS

IV.A. Descriptive Statistics and Control Group

Table 1 summarizes the baseline descriptive features for the three expansion states compared to a) the propensity-defined control group; b) the four neighboring states used as the

[11] See Appendix Table 1 in Sommers et al. (2014) for the full list of diagnosis codes.

original control group from Sommers et al. 2012 (Pennsylvania, Nevada, New Mexico, and New Hampshire); and c) the full U.S. sample outside of the expansion states. Given the large numbers of counties (n=1000, for instance, in comparing the treatment and control groups), many of these demographic differences are statistically significant, but here I focus on the absolute magnitude of those differences as the more relevant measure of whether the groups are reasonably comparable.[12]

The largest differences between the expansion states and the neighboring state control group were for race/ethnicity and the poverty rates. On all measures but one (median income), the propensity score control group resembles the expansion states more closely than does the neighboring state control group. For instance, 15.9% of the population in the Medicaid expansion states is Latino, far higher than the 8.3% figure in the neighboring state sample; but it is 15.7% in the propensity score control group. Similarly, the poverty rate in the neighboring state sample was 10.8%, while it was 12.9% in the propensity score control and 14.2% in the expansion states. There are moderate level differences in baseline mortality rates – 318 per 100,000 in the expansion state, vs. 297 in the propensity score group and 343 in the neighboring states. The following section presents results of the tests for pre-expansion mortality trends.

IV.B. Pre-expansion Trends and Placebo Tests

Figure 1 depicts mortality changes in the Medicaid expansion states and the propensity-score control group (hereafter referred to simply as "the control group") for all-cause mortality, deaths due to healthcare-amenable causes, and deaths from other causes. The pre-expansion mortality curves followed similar trajectories prior to 2001, when the expansions began. In the

[12] For instance, the percentage of adults in the 34-45 age group was 27.1% in the expansion states and 27.5% in the propensity score control group, p=0.03, but in the context of a differences-in-differences analysis which also adjusts directly for age, this 0.4 percentage-point difference is highly unlikely to bias the results.

post-expansion period, mortality in the expansion states fell while it rose slightly in control states. Healthcare-amenable deaths show a post-2001 decline in expansion states, but not in control states. For non-amenable (or, to be more accurate, "less amenable") causes of death, mortality rose slightly in control states after 2001 and was relatively flat in the expansion states.[13]

Table 2 presents the regression results for tests of parallel trends and the placebo test for the year 2000, comparing expansion states and the control group. In the linear trend analysis, the coefficient on *Expansion State*Time Trend* was statistically non-significant and close to zero (0.7 per 100,000 per year, p=0.56). In the placebo differences-in-differences model, the coefficient on *Expansion State*Year2000* was also small and non-significant (1.8 per 100,000, p=0.42). Thus both the time trend and placebo analyses show that the propensity score approach yielded a good match in baseline trend, consistent with the visual evidence in Figure 1. The negative binomial GLM model produced similarly reassuring null results.

IV.C. Changes in All-Cause Mortality

Table 3 presents the differences-in-differences results for all-cause mortality, in linear and negative binomial GLM models. In both models, Medicaid expansions led to a significant decline in all-cause mortality, equivalent to 19.1 fewer deaths per 100,000 in the linear model and a relative decline of 6.0% in the negative binomial model. These two results are nearly identical in comparison to the baseline mortality rate of 318 per 100,000. Other significant predictors of mortality were older age, black race, male sex, and higher poverty / lower median income.[14] The point estimates here are essentially unchanged from those in the original Sommers et al. (2012)

[13] The impact of the terrorist attacks of September 11th are visible in the non-amenable causes of death, with a slight uptick in this outcome for expansion states in 2001, before the mortality rate returns to its 2000 level the following year.
[14] See footnote 5 for further discussion of the effects of economic indicators on mortality.

analysis that used a control group of neighboring states and found mortality reductions of 19.6 per 100,000, a relative 6.1% decrease.

IV.D. Analyses by Cause of Death and HIV

IV.D.1: Healthcare-Amenable Mortality

Table 4 presents the differences-in-differences estimates for several types of mortality based on cause of death. Mortality reductions were highly significant ($p \leq 0.01$) for healthcare-amenable deaths, with a linear estimate of -12.0 per 100,000 and the negative binomial model showing a 6.7% reduction after the Medicaid expansion. Non-amenable deaths also declined, though the effects were only marginally significant in both models ($p \leq 0.10$). While generally supportive of the hypothesis that healthcare-amenable diagnoses should be more highly impacted than other causes of death, the estimates for non-amenable causes are fairly imprecise and contain 95% confidence intervals extending well above and below the 6.0% relative decline in all-cause mortality.

IV.D.2: HIV-Related Mortality

Figure 2 plots HIV-specific mortality during the study period for the expansion states and the control group. HIV-related mortality in the expansion states was nearly three-fold higher than in the control states in the baseline period, and was already declining in both treatment and control states prior to the Medicaid expansions. However, the decline in control states was gradual after 2002, while the decline continued to accelerate in the expansion states, markedly narrowing the gap compared to the control states.

The regression results in Table 4 shows that HIV-related deaths were indeed declining more rapidly in the treatment states after Medicaid expansion, by 3.8 deaths per 100,000 in the linear model. However, the linear model is biased by the difference in absolute risk of HIV at baseline, and pre-expansion trends show a significant divergence in HIV mortality (coefficient on *Expansion State*Time Trend* = -0.9, $p<0.001$). This bias in the absolute HIV-mortality reduction does not require that there was any faster diffusion of antiretrovirals in expansion states than non-expansion states; the large difference in baseline prevalence of HIV alone creates this bias in the linear differences-in-differences model, even if the relative mortality reduction from these treatments was identical across states. The linear estimate in Table 4 suggests that roughly 20% (3.8/19.1) of the all-cause mortality decline after Medicaid expansions were attributable to HIV. Even after removing HIV deaths from the analysis, mortality from all other causes still experienced a highly significant decline in both models. The most cautious reckoning of this result is to assume that the full HIV effect was solely due to the introduction of antiretroviral medications and had nothing to do with Medicaid expansion.

However, this assumption is likely inaccurate, as insurance coverage may have an interactive effect with new medical technology – especially for expensive treatments such as antiretrovirals. While federals funding through the Ryan White Program provides access to HIV-related treatment to many low-income Americans, it does not provide comprehensive insurance and is subject to block grants to states and localities that may limit its reach (Kaiser, 2013). This suggests a significant potential role for Medicaid to expand access to HIV treatment, consistent with one prior instrumental variables analysis of insurance coverage in the early years of antiretroviral medications, which detected a 70-85% reduction in mortality for HIV-positive individuals obtaining Medicaid (Goldman et al., 2001).

For measuring whether Medicaid expansion facilitated the mortality reductions from antiretroviral therapy, the negative binomial GLM approach is preferable to the linear model, since it measures the differential *relative* change in HIV mortality and is not biased by higher baseline absolute HIV mortality in the expansion states. In the GLM model, there was no significant divergence in pre-expansion trends (-0.013, p=0.13), and the differences-in-differences estimate indicates a 13.6% relative reduction in HIV mortality due to the Medicaid expansion. The unadjusted mortality statistics for HIV tell a similar story: the pre vs. post-expansion HIV death rate went down by 16% in the control group, while it decreased by 34% in expansion states. This suggests that Medicaid expansion facilitated the impact of antiretrovirals in the treatment of HIV, consistent with Goldman et al.'s 2001 findings.

As an additional analysis to probe these results, I created an HIV-specific comparison group, using the same matching approach described earlier, but with a focus on HIV-related mortality in the pre-expansion period. Compared to the top quartile of counties matched on pre-expansion HIV mortality patterns and demographics, Medicaid expansion led to a 15.4% relative reduction in HIV-related deaths (p=0.007) in the GLM model, and an absolute reduction in the linear model of -2.6 per 100,00 from a baseline mean of 17/100,000 (p=0.011), similar to the results in Table 4. However, even within this HIV-specific control group, there was evidence of divergence in the pre-expansion HIV-mortality trends (-1.0, p<0.001). Narrowing the control group to the top decile of propensity-matched counties produced similar divergence. The difficulty in finding a comparable control group for this outcome points to how atypical the expansion states (New York in particular) were in terms of their high HIV rates.

IV.E. Analyses Excluding New York

Since Sommers et al. (2012) found that significant mortality changes were limited to New York (by far the largest expansion state in the study sample), I also tested the impact of excluding New York from our sample, and using a propensity-matched control group for Maine and Arizona constructed analogously to the overall control group. Table 5 presents these results. The estimated mortality declines in both linear and GLM models were much smaller than for the overall sample, consistent with the previous finding that New York's mortality changes were the primary driver for the study results. With a more refined control group and richer data on case of death, I do find a significant decline of 2.7% in healthcare-amenable deaths in these two states in the negative binomial model. However, this finding is contingent on the functional form and is not significant in the linear model.[15] While suggestive of a potential impact of the Medicaid expansion in these two states, mortality changes in these states nonetheless appear smaller than those in New York.

Numerous policy differences exist between Medicaid programs across states, including covered benefits, provider adequacy, and quality of care. While it is possible that the New York Medicaid expansion produced larger mortality changes because of differences in program features, a more likely explanation is that the much smaller populations in Maine and Arizona, combined with smaller relative expansions, limited the power to detect a population-level effect on mortality. Enrollment statistics for the Medicaid expansion programs, available from winter 2005, indicated that New York had enrolled over 460,000 individuals in its expansion (Artiga & Mann, 2005) – approximately 4% of its non-elderly adult population, based on data from the Current Population Survey. By contrast, Maine had enrolled just under 24,000 (2.8% of the 19-64 age group) and Arizona only 13,000 (less than 1% of this age group). Given these dramatic differences in

[15] Further limiting the strength of this finding is that the likelihood of a significant result is increased due to the multiple comparisons of two distinct models (linear and negative binomial). A basic Bonferroni correction for these two models would require reducing the significance threshold by 50%. The p-value on the GLM estimated reduction in health-care amenable deaths was 0.029, implying that after adjusting for the two models, it would remain significant at the $p<0.10$ level, but not $p<0.05$.

population size and expansion impact, it is not surprising that the results for the two smaller states are much more equivocal.

Another possibility, of course, is that there was something other than the coverage expansion driving the changes in New York not present elsewhere. To address these concerns, the next section considers several robustness checks for whether the Medicaid expansions and resulting increases in insurance coverage were indeed the source of these mortality reductions.

V. ALTERNATIVE SPECIFICATIONS AND COST-BENEFIT ANALYSIS

V.A. DDD Model Using Elderly Adults as Additional Control

One natural test of whether Medicaid expansion was in fact the underlying cause for the observed mortality decline comes from a comparison to elderly adults. Adults 65 and over are generally excluded from Medicaid eligibility expansions approved by the federal government for low-income adults, and moreover, baseline coverage rates are in the range of 98-99% due to the presence of Medicare (DeNavas-Walt, Proctor, & Smith, 2011). However, many potential state-specific mortality changes related to improvements in medical technology, health care infrastructure, the environment, or epidemiology of disease – all of which might differentially impact non-elderly mortality in the expansion states compared to the control states – should be filtered out by the DDD approach.

The model was a standard DDD regression, described in Equation 5:

$$\begin{aligned}(5)\qquad Deaths_{ijklt} = {} & \beta_0 + \beta\,\mathbf{X_{ijk}} + \beta_1\,Post\text{-}Expansion_t * Non\text{-}Elderly_i \\ & + \beta_2\,Non\text{-}Elderly_i * ExpansionState_s \\ & + \beta_3\,ExpansionState_s * Post\text{-}Expansion_t \\ & + \beta_4\,ExpansionState_s * Post\text{-}Expansion_t * Non\text{-}Elderly_i\end{aligned}$$

$$+\beta_5 \textbf{County-Level Factors}_{\textbf{ct}} + \mu\, Year_t + \Omega\, State_s + \varepsilon_{ijklst}$$

Direct effects for age are captured by the vector **X** (which includes age group, race, and sex), while direct effects of the post-expansion period and being in an expansion state are captured by μ, a vector of year fixed effects, and Ω, a vector of state fixed effects. County-level factors are as in Equation 4. The full set of interaction terms for age, time period, and expansion state are included. β_4 is the coefficient of interest, capturing the effect of being in an expansion state in years after Medicaid expansion for non-elderly adults, relative to adults 65 and older. Given the markedly different baseline death rates for elderly versus non-elderly adults, this analysis uses the negative binomial GLM, which reports the relative decline in mortality.[16]

Figure 3 shows the mortality trends for elderly adults in expansion and control states, and in contrast to the findings in Figure 1, the trends for all-cause, healthcare-amenable, and non-amenable deaths appear similar before and after the Medicaid expansions. Table 6 presents the regression results. All-cause mortality changed by -1.5% among elderly adults after Medicaid expansion in the treatment states, while the DDD estimate for non-elderly adults was a -4.8% decline after the expansion, with both coefficients significant at $p<0.10$. This suggests that there may have been a small secular trend towards declining death rates for both older and younger adults in the expansion states, but the mortality effect specific to non-elderly adults was more than 4 times larger (6.3% vs. 1.5%). The pattern for amenable versus non-amenable causes of death provides additional support for Medicaid expansion as the causal mechanism: the DDD estimate for healthcare-amenable mortality among non-elderly adults was -6.2% ($p<0.001$), with

[16] The original Sommers et al. (2012) analysis conducted a similar DDD, using the log of mortality. That analysis yields similar results as the negative binomial GLM, though requires using the log of the death rate + 1, since numerous cells (particular for young adults in small counties) have 0 deaths. The negative binomial model handles these zero values without difficulty and with a better distributional fit for count data.

no direct effect on elderly adults, and neither age group experienced a statistically significant decline in non-amenable causes of death after the Medicaid expansion.

V.B. Mortality and County-Level Changes in the Uninsured Rate

In this section, I test directly whether county-level changes in health insurance rates after the Medicaid expansion were tied to the observed mortality changes, similar to several analyses of Massachusetts' health reform (Miller, 2012; Sommers, Long, et al., 2014). These analyses use county-level data on the percentage of adults 19-64 who are uninsured, from the U.S. Census Bureau's Small Area Health Insurance Estimates (SAHIE). SAHIE data are available for 2000-2001 (the pilot years of the program), and subsequently from 2005 onwards, which means data are not available for multiple pre- and post-expansion years in the study period. I present several analyses to account for these data limitations. In addition, the baseline 2000 SAHIE data were for adults ages 19 and older. To make these estimates comparable to the 2005-2007 figures (ages 19-64), I subtracted out the elderly population of each county, assuming that the elderly coverage rate was 100%. To the extent that this introduces measurement error, it would likely bias the estimated impact of coverage on mortality towards the null.

The first approach simply stratifies the original sample into two sets of counties – high uninsured counties and low-uninsured counties, using the 2000 population-weighted median uninsured rate in the expansion states. The results (Table 7) indicate that counties with high uninsured rates prior to Medicaid expansion experienced much larger mortality reductions (-29.4 per 100,000), compared to counties with low uninsured rates (-7.7 per 100,000). These baseline uninsured rates are a reasonably good proxy for the impact of Medicaid expansions: comparing the

2000 baseline rate to the net county-level increase in coverage from 2000-2007 in the SAHIE yields a correlation coefficient of 0.58.

Next, I estimate the following linear model using those years for which SAHIE data are available (2000, 2005-2007), replacing the binary differences-in-differences indicator with a parameterized measure of coverage gains from the Medicaid expansion:

$$(6) \qquad MortalityRate_{ijkct} = \beta_0 + \beta_1 \mathbf{X_{ijk}} + \beta_2 \, \mathbf{County\text{-}Level\ Factors_{ct}} + \beta_3 \Delta Coverage_{ct} + \beta_4 \, ExpansionState_s {*} \Delta Coverage_{ct} + \mu \; Year_t + \Omega \; County_c + \varepsilon_{ijkct}$$

Δ*Coverage* represents the change in county-level uninsured rate (expressed as a positive gain in coverage) compared to 2000. For data from 2000 or earlier, it is equal to 0. β_4 is the coefficient of interest, measuring the impact of coverage gains in Medicaid expansion states. All other variables are defined as in Equation 4. I also consider a model limiting the data to a single pre- and post-expansion data point (2000 and 2007) for a balanced panel, or alternatively, using the full study period with imputed values for county-level uninsured rates in years without any SAHIE estimates based on linear extrapolation between the 2000 and 2005 data for the intervening years, and assuming that pre-expansion uninsured rates were flat. I also estimate the same set of models with the addition of the original *ExpansionState * Post* interaction term, allowing for changes in mortality in expansion states not mediated by changes in the county-level uninsured rate, given previous findings of positive spillover effects of coverage expansion even on those who already have health insurance (Pauly & Pagan, 2007).[17]

[17] An alternative approach, assuming that Medicaid expansion only impacted mortality via changes in the county-level uninsured rate, would be to use Medicaid expansion as an instrument for county-level changes in the uninsured rate. This approach yields an even larger reduction in mortality linked to coverage gains, with a coefficient of -1127 (p=0.04). However, the results in Table 7 indicate that the direct effect of *ExpansionState*Post* remains significant in models containing county-level coverage changes, which suggests that the SAHIE county-level estimates of coverage gains do not explain the full mortality effect. This may be due to a combination of measurement error in

Table 7 presents these results. In all models, county-level coverage gains led to significant mortality declines, with point estimates ranging from -316 to -419. Δ*Coverage* is reported as a fraction from -1.0 to 1.0, so these coefficients indicate that each percentage point of coverage led to 3.16-4.19 fewer deaths per 100,000. Alternatively, this suggests that the expansions needed to cover between 239 and 316 people to prevent one death per year, the so-called number-needed-to-treat (NNT).[18] This range falls between the NNT of 176 estimated in Sommers, Baicker, and Epstein's 2012 analysis of these Medicaid expansions, which used administrative enrollment figures for their calculation, and the NNT of 830 in Sommers, Long, and Baicker's 2014 analysis of Massachusetts' health reform.

We can use this parameter to translate the observed population level mortality changes into an individual-level risk reduction in mortality due to gaining coverage, with the following relationship:[19]

*NNT * baseline mortality * individual risk reduction = 1 death per 100,000.*

Using a NNT of roughly 300, based on the estimates in Table 7, this leaves two degrees of freedom: the baseline mortality rate for those gaining insurance, and the individual-level risk reduction of coverage. Neither is observed directly in the mortality dataset, but one can make reasonable inferences for the former. The baseline population-wide mortality rate in expansion states was 318/100,000. But the mortality rate for uninsured adults – and in particular for uninsured adults living near or below the federal poverty level – is significantly higher. The

---

the SAHIE and that the expansions also impact mortality through channels other than increasing coverage rates, such as health impacts of shifting from private to public coverage, potential spillovers from bringing in more resources to the health care system, or other factors (see Pauly & Pagan, 2007). In any event, these results suggest that the exclusion restriction does not hold and the IV analysis is therefore not appropriate for determining the causal effect of increases in insurance coverage.

[18] A coefficient of -419 means -4.19 fewer deaths per 100,000 for a percentage point of coverage. NNT = 1000 covered per 100,000 / 4.19 deaths prevented per 100,000 = 239.

[19] This calculation makes the assumption that all mortality gains accrue to those individuals who gained coverage, without any spillover effects on other people in the state. If there are any spillover effects on population mortality, this calculation will overstate the individual-level risk reduction associated with acquiring health insurance.

relative risk of death among poor adults ages 19-64, compared to the general population, has been estimated to be 1.75 (Galea, Tracy, Hoggatt, Dimaggio, & Karpati, 2011). Even after controlling for income, sex, and age, the uninsured still have higher mortality rates, with a relative risk estimated by Kronick (2009) to be 1.25. Putting these two hazard ratios together with the baseline mortality rate implies that low-income uninsured adults in these states had a mortality rate of roughly 700 per 100,000. If we assume that the mortality rate for those who signed up for Medicaid – conditional on being uninsured and poor – was equal to those who did not sign up for Medicaid, this implies an individual-level mortality reduction of 48%.

However, previous research has demonstrated that poor health itself is a strong predictor of Medicaid take-up, conditional on eligibility (Kenney, Lynch, Haley, & Huntress, 2012; Sommers, Kenney, & Epstein, 2014). This suggests that baseline mortality among those who actually enrolled in these states' Medicaid expansions was likely significantly higher. If adverse selection confers an additional 25% mortality risk,[20] this would imply an individual-level mortality reduction from gaining insurance of 38%, consistent with several prior estimates (Goodman-Bacon, 2013; Hadley, 2003; Wilper et al., 2009) from the admittedly conflicting literature on the topic. If we use the lowest bound from the 95% confidence intervals associated with Table 7, the individual-level risk reduction would be 25%.[21]

V.C. Additional Robustness Checks

[20] The extent of adverse selection could actually be much larger than this, given that Medicaid enrollment often occurs at the point-of-care for active medical issues, in doctor's offices, emergency departments, and hospitals.

[21] An analogous calculation using the NNT in the Massachusetts' health reform analysis by Sommers, Long, Baicker (2014) yields somewhat similar results. The Massachusetts NNT was 830, with a baseline mortality rate of 283 per 100,000. However, unlike the Medicaid expansions, the Massachusetts coverage expansion was not limited to low-income individuals and, due to the individual mandate, likely experienced far less adverse selection. Kronick (2009) estimates a 1.7 elevated mortality risk for all uninsured adults, adjusted only for age and sex. This yields a baseline mortality for newly covered adults of roughly 480 per 100,000, which combined with the NNT, implies a 25% individual-level risk reduction from gaining insurance.

Appendix Table A.1 presents several additional robustness checks of the primary analyses of all-cause mortality presented in Table 3. Pooling the data into a collapsed pre- and post-period to avoid any potential serial autocorrelation (Bertrand et al., 2004) produces nearly identical results, as does collapsing the county data into 500 state-year cells. Adding the year 2001 back into the sample also has minimal impact on the results. An interrupted times-series model, in which the expansion is modeled as a change in slope in the mortality rate instead of an average change in level, shows that mortality gains grew over time: each additional post-expansion year led to a decline in the death rate of 4.3 per 100,000 ($p<0.001$).

V.D. Cost-Benefit Analysis

The results in Table 7 suggest a fairly straightforward way to assess the relative cost-benefit ratio of Medicaid's impact on population mortality. In Section V.B., I estimated that the Medicaid expansion covered 239-316 adults to prevent one death per year. The Oregon Health Insurance Experiment, studying a similar population of low-income adults eligible for a Medicaid expansion, detected a 25% increase in overall medical spending from gaining Medicaid, equal to an estimated $778 increase from a baseline mean of $3156 in 2007 dollars (Finkelstein et al., 2012).[22] Thus, each death prevented in this analysis can be linked to an estimated increase in direct health care spending of $186,000 to $246,000, based on the number-needed-to-treat.[23]

[22] See LATE results in Table V from Finkelstein et al. (2012). While New York has the nation's most expensive Medicaid program, this is mostly due to costs for elderly adults and individuals with disabilities. The OHIE estimate for per-person spending among non-elderly adults in Medicaid ($3934) is quite similar to a contemporary estimate of per capita costs for non-elderly adults in New York's Medicaid program: $3953 in 2003 dollars, equal to $4454 in 2007 inflation-adjusted terms (see Figure 4 in: http://www.cbcny.org/sites/default/files/reportsummary_medicaid_04202006.pdf). Replacing the baseline level of spending in Equation 7 from OHIE with the New York specific estimate simply increases the results in Table 8 proportionately, i.e. by 13%.

[23] To be consistent with the previous calculation of the individual-level risk reduction in mortality, this analysis makes the assumption that all mortality gains accrue to those individuals who gained coverage, without any spillover effects. However, if one assumes that any potential spillover effects are mediated by the added health care spending

However, this does not account for the fact that Medicaid expansion replaces with tax-supported insurance coverage the other 75% of medical spending that was primarily occurring using private dollars – either out of pocket spending or uncompensated care. While some of this pre-expansion care was likely publicly-financed to begin with, via subsidized care at public hospitals and clinics, I make the conservative assumption that it was all privately funded – which means that these results provide an upper bound on the total cost per life saved. Factoring in the deadweight loss of this crowd-out of private medical spending (even though there is no evidence that these expansions crowded out private *insurance*), we are left with the following formula for cost per life saved:

$$\textit{Cost per Life Saved} = NNT * [\$778 * (1+DWL) + \$3156 * DWL]$$

where *NNT* is number needed to treat and *DWL* is the deadweight loss of public financing. Typical estimates in the public economics literature for DWL range from 15% to 50% (Ballard, Shoven, & Whalley, 1985; Browning, 1987). This range, combined with the set of values for NNT estimated above, produces a cost per life saved ranging from $327,000 to $867,000 (Table 8) in 2007 dollars, the last year of the study period.[24] These values compare favorably to estimates from the literature on the value of a statistical life (Viscusi, 1992, 1993) used by the government to evaluate public health and environmental policies. Based largely on the work of Kip Viscusi, the federal government's estimates for this value have been pegged at a mean of $7.6 million, with a range from $950,000 to $21.4 million, in 2007 inflation-adjusted dollars (Robinson, 2007).

---

associated with Medicaid expansion, then the cost-benefit calculation would be identical whether or not all the deaths prevented were among those gaining coverage directly versus via spillover impacts of the expansion.

[24] Several back-of-the-envelope cost-benefit analyses have been advanced following the publication of the Massachusetts mortality study (Cannon, 2014; Pollack, 2014). However, these typically have treated the government payment towards subsidized coverage as the "cost of insurance." This is incorrect for two reasons. First, we are interested in the marginal societal cost, and in the absence of subsidized coverage uninsured people do not consumer zero health care – in fact, they appear to consume nearly 80% of what insured people do, according to the OHIE. Second, this approach also ignores the deadweight loss of public financing for subsidized insurance. The first omission is much larger than the second, which means that these previous estimates overstate the cost-per-life compared to the method used here.

Of course, Table 8 reflects only the benefits of coverage directly related to mortality reductions. Health-related quality of life and financial protection are far more common benefits of coverage. Previous analyses have suggested a utility-based gain from financial risk protection in Medicaid equivalent to a value ranging from roughly $500 to $3000 per person, depending on the extent of risk aversion (Sommers & Oellerich, 2013). Meanwhile, OHIE found that Medicaid increased the proportion of adults reporting "good, very good, or excellent health" by 13 percentage points (Finkelstein et al., 2012) and reduced depression by 9 percentage points (Baicker et al., 2013). A cost-benefit analysis accounting for these changes would render the economic evaluation of Medicaid even more favorable.[25]

VI. Conclusion

In this differences-in-differences analysis of state Medicaid expansions to low income adults, I find that expansions led to a 6% relative decline in mortality over five years of follow-up, compared to a control group of counties with similar pre-expansion mortality trends and population demographics. There was a highly significant decline in healthcare-amenable causes of death, while effects for non-amenable causes were generally smaller and only marginally statistically significant. Declines in HIV-related deaths accounted for 20% of the overall mortality effect, and the Medicaid expansions appear to have facilitated the impact of antiretroviral therapy during this period. Overall mortality reductions were largest in New York, the state with the largest expansion, though I find suggestive evidence that healthcare-amenable mortality declined in the other two states. Mortality declines were closely linked to county-level

[25] Incorporating changes in self-reported health and depression into a cost-benefit framework would require conversion into a quality-adjusted life year or some other per-year monetarization of benefits. To incorporate those benefits into a similar equation as the mortality reductions estimated here would also require more detailed information as to the age of the would-be decedents whose deaths have been prevented, in order to assess the likely added longevity. Both of these exercises would be fairly speculative and beyond the scope of the current paper.

changes in insurance coverage, with one death prevented annually for every 239 to 316 adults gaining health insurance, which implies a cost per life saved that compares favorably to the standards use to evaluate existing public policy interventions.

REFERENCES

Abadie, A., Diamond, A., & Hainmueller, J. (2012). Comparative Politics and the Synthetic Control Method. Cambridge, MA: Harvard Kennedy School.

Artiga, S., & Mann, C. (2005). Coverage gains under recent Section 1115 waivers: a data update. Washington, D.C.: Kaiser Family Foundation.

Baicker, K., Taubman, S., Allen, H., Bernstein, M., Gruber, J., Newhouse, J. P., . . . Group, O. H. S. (2013). The Oregon Experiment - Effects of Medicaid on Clinical Outcomes. *The New England journal of medicine, 368*(18), 1713-1722.

Ballard, C. L., Shoven, J. B., & Whalley, J. (1985). General equilibrium computations of the marginal welfare costs of taxes in the United States. *American Economic Review, 75*, 128-138.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How Much Should We Trust Differences-in-Differences Estimates? *Quarterly Journal of Economics, 119*, 249-275.

Black, B., Espin-Sanchez, J. A., French, E., & Litvak, K. (2013). The Effect of Health Insurance on Near-Elderly Health and Mortality *Social Science Research Network*.

Brown, G. K., Beck, A. T., Steer, R. A., & Grisham, J. R. (2000). Risk factors for suicide in psychiatric outpatients: a 20-year prospective study. *Journal of consulting and clinical psychology, 68*(3), 371-377.

Browning, E. K. (1987). On the marginal welfare cost of taxation. *American Economic Review, 77*, 11-23.

Cannon, M. (2014). New Study Suggests RomneyCare Saved Lives, but at a Very High Cost: Forbes.com.

Card, D., Dobkin, C., & Maestas, N. (2009). Does Medicare Save Lives? *Quarterly Journal of Economics, 124*(2), 597-636.

Currie, J., & Gruber, J. (1996a). Health insurance eligibility, utilization of medical care and child health. *Quarterly Journal of Economics, 111*(2), 431-466.

Currie, J., & Gruber, J. (1996b). Saving Babies: The Efficacy and Cost of Recent Expansions of Medicaid Eligibility for Pregnant Women. *Journal of Political Economy, 104*(6), 1263-1296.

DeLeire, T., Dague, L., Leininger, L., Voskuil, K., & Friedsam, D. (2013). Wisconsin Experience Indicates that Expanding Public Insurance to Low-Income Childless Adults Has Health Care Impacts. *Health affairs, 32*(6).

DeNavas-Walt, C., Proctor, B., & Smith, J. (2011). Income, poverty, and health insurance coverage in the United States: 2010. Washington, DC: U.S. Census Bureau.

Doyle, J. J. (2005). Health insurance, treatment and outcomes: using auto accidents as health shocks. *Review of Economics and Statistics, 87*(2), 256-270.

Duggan, M. G., & Evans, W. N. (2008). Estimating the Impact of Medical Innovation: A Case Study of HIV Antiretroviral Treatments. *Forum for Health Economics & Policy, 11*(2), Article 1.

Epstein, A. M., & Newhouse, J. (1998). The impact in Medicaid expansion for pregnant women on early prenatal care, use of cesarean section and maternal and infant health outcomes in South Carolina and California. *Health Care Finance Review, 19*(4), 85-99.

Finkelstein, A., & McKnight, R. (2008). What did Medicare do? The initial impact of Medicare on mortality and out of pocket medical spending. *Journal of Public Economics, 92*, 1644-1668.
Finkelstein, A., Taubman, S., Wright, B. J., Bernstein, M., Gruber, J., Newhouse, J. P., . . . Group, O. H. S. (2012). The Oregon Health Insurance Experiment: Evidence from the First Year. *Quarterly Journal of Economics, 127*(3), 1057-1106.
Galea, S., Tracy, M., Hoggatt, K. J., Dimaggio, C., & Karpati, A. (2011). Estimated deaths attributable to social factors in the United States. *American journal of public health, 101*(8), 1456-1465.
Goldman, D., Bhattacharya, J., McCaffrey, D., Duan, N., Leibowitz, A., Joyce, G., & Morton, S. (2001). The Effect of Insurance on Mortality in an HIV+ Population in Care. *Journal of the American Statistical Association, 96*(455), 883-894.
Goodman-Bacon, A. (2013). Public Insurance and Mortality: Evidence from Medicaid Implementation. Ann Arbor, MI: University of Michigan Working Paper.
Hadley, J. (2003). Sicker and poorer: the consequences of being uninsured. *Medical Care Research and Review, 60*(2), 3S-75S.
Howell, E. M. (2001). The impact of the Medicaid expansions for pregnant women: a synthesis of the evidence. *Medical care research and review : MCRR, 58*(1), 3-30.
Kaestner, R. (2012). Mortality and access to care after Medicaid expansions. *The New England journal of medicine, 367*(25), 2453-2454; author reply 2454.
Kaiser. (2013). The Ryan White Program. Washington, DC: Kaiser Family Foundation.
Kenney, G. M., Lynch, V., Haley, J., & Huntress, M. (2012). Variation in Medicaid eligibility and participation among adults: implications for the Affordable Care Act. *Inquiry : a journal of medical care organization, provision and financing, 49*, 231-253.
Kronick, R. (2009). Health insurance coverage and mortality revisited. *Health services research, 44*(4), 1211-1231.
Long, S. K., Coughlin, T., & King, J. (2005). How well does Medicaid work in improving access to care? *Health services research, 40*(1), 39-58.
Miller, S. (2012). The effect of insurance on emergency room visits: An analysis of the 2006 Massachusetts health reform. *J Pub Econ, 96*(11-12), 893-908.
Nolte, E., & McKee, C. M. (2008). Measuring the health of nations: updating an earlier analysis. *Health affairs, 27*(1), 58-71.
Nolte, E., & McKee, M. (2003). Measuring the health of nations: analysis of mortality amenable to health care. *BMJ, 327*(7424), 1129.
Palella, F. J., Jr., Baker, R. K., Moorman, A. C., Chmiel, J. S., Wood, K. C., Brooks, J. T., & Holmberg, S. D. (2006). Mortality in the highly active antiretroviral therapy era: changing causes of death and disease in the HIV outpatient study. *Journal of acquired immune deficiency syndromes, 43*(1), 27-34. doi: 10.1097/01.qai.0000233310.90484.16
Pauly, M. V., & Pagan, J. A. (2007). Spillovers and vulnerability: the case of community uninsurance. *Health affairs, 26*(5), 1304-1314.
Pollack, H. (2014). Could Obamacare save 24,000 lives a year? : Healthinsurance.org.
Robinson, L. A. (2007). How US Government Agencies Value Mortality Risk Reductions. *Review of Environmental Economics and Policy, 1*(2), 283-299.
Roy, A. (2013). *How Medicaid Fails the Poor*. Jackson, TN: Encounter Books.
Ruhm, C. J. (2005). Commentary: mortality increases during economic upturns. *International journal of epidemiology, 34*(6), 1206-1211.

Ruhm, C. J. (2009). Economic conditions and health behaviors: are recessions good for your health? *North Carolina medical journal, 70*(4), 328-329.

Sommers, B. D., Baicker, K., & Epstein, A. M. (2012). Mortality and access to care among adults after state Medicaid expansions. *The New England journal of medicine, 367*(11), 1025-1034. doi: 10.1056/NEJMsa1202099

Sommers, B. D., Kenney, G. M., & Epstein, A. M. (2014). New evidence on the affordable care act: coverage impacts of early medicaid expansions. *Health affairs, 33*(1), 78-87. doi: 10.1377/hlthaff.2013.1087

Sommers, B. D., Long, S. K., & Baicker, K. (2014). Changes in Mortality After Massachusetts Health Care Reform: A Quasi-experimental Study. *Annals of internal medicine, 160*(9), 585-593. doi: 10.7326/M13-2275

Sommers, B. D., & Oellerich, D. (2013). The poverty-reducing effect of Medicaid. *Journal of health economics, 32*(5), 816-832.

Viscusi, W. K. (1992). *Fatal Trade-offs: Public and Private Responsibilities for Risk*. New York: Oxford University Press.

Viscusi, W. K. (1993). The value of risks to life and health. *Journal of Economic Literature, 31*, 1912-1946.

Wilper, A. P., Woolhandler, S., Lasser, K. E., McCormick, D., Bor, D. H., & Himmelstein, D. U. (2009). Health insurance and mortality in US adults. *American journal of public health, 99*(12), 2289-2295.

TABLE 1: Baseline Features (1997-2000) of Medicaid Expansion States, Alternative Control Groups, and All Non-Expansion States

Variable	Expansion States	PS Control	Neighboring Control	Rest of U.S.
Age 20-24	11.0%	10.6%	10.5%	11.3%
Age 25-34	25.3%	25.3%	23.3%	24.9%
Age 35-44	27.1%	27.5%	27.8%	27.5%
Age 45-54	21.9%	22.0%	23.0%	22.0%
Age 55-64	14.7%	14.6%	15.4%	14.2%
Male	49.0%	49.2%	49.5%	49.7%
White	79.2%	81.2%	88.3%	82.7%
Black	14.0%	10.7%	8.1%	12.2%
Other Race	6.9%	8.0%	3.6%	5.1%
Latino Ethnicity	15.9%	15.7%	8.3%	11.1%
Poverty Rate	14.2%	12.9%	10.8%	12.0%
Median Household Income	$53,866	$57,880	$53,629	$55,150
Unemployment Rate	5.3%	4.8%	4.6%	4.4%
Mortality (per 100,000)	318	297	343	330
Number of counties	93	907	127	3047
Number of states	3	47	4	48

Notes:
Medicaid expansion states were New York, Arizona, and Maine. PS control is the propensity-score defined control group. Neighboring control is the original four-state control group (Pennsylvania, Nevada, New Mexico, and New Hampshire) used in Sommers, Baicker, and Epstein (2012), which were demographically similar states neighboring the expansion states. Sample contains adults ages 20-64. “Rest of U.S.” includes 47 states and the District of Columbia.

TABLE 2: Tests of Pre-Expansion Trends and Placebo Tests for All-Cause Mortality, 1997-2000

Specification	Linear Model	Negative Binomial GLM
Linear Trend (Equation 2):	0.69	-.0064
*Expansion State*Time Trend*	(1.18)	(.0063)
Placebo Test (Equation 3):	1.80	-.0096
*Expansion State * Year2000*	(2.21)	(.0145)

Notes: All coefficients non-significant at p>0.10. Linear model reports changes in deaths per 100,000. Negative binomial generalized linear model (GLM) reports relative change in mortality. Sample contains adults ages 20-64.

TABLE 3: Effect of State Medicaid Expansions on All-Cause Mortality

Model and Variable	Linear Model	Negative Binomial GLM
*Expansion State * Post-Expansion*	-19.1***	-.060***
	(6.3)	(.023)
Age 25-34	12.2***	.085***
	(2.5)	(.027)
Age 35-44	98.6***	.79***
	(5.7)	(.05)
Age 45-54	311.3***	1.60***
	(9.9)	(.05)
Age 55-64	778.6***	2.42***
	(17.7)	(.06)
Male	172.6***	.68***
	(7.0)	(.01)
White	80.8***	.53***
	(18.8)	(.10)
Black	268.9***	1.10***
	(22.5)	(.10)
% Hispanic	0.38	-.006***
	(0.92)	(.002)
Poverty Rate	1.35***	.014***
	(0.41)	(.004)
Median Household Income	-0.001***	-.000006***
	(0.0004)	(.000002)
Unemployment Rate	-1.25	.009
	(0.93)	(.006)

Notes:
* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$
Linear model reports changes in deaths per 100,000. Negative binomial generalized linear model (GLM) reports relative change in mortality. Robust standard errors, clustered at the state-level (n=50), are in parentheses. All models include year fixed effects. Linear model also includes county fixed effects, and GLM includes state fixed effects. See Equation 4 in text for full details. Sample contains adults ages 20-64.

TABLE 4: Effect of State Medicaid Expansions on Mortality, By Cause of Death

Cause of Death	Pre-Expansion Mean	Linear Model	Negative Binomial GLM
All-Cause Mortality	318	-19.1***	-.060***
		(6.3)	(.023)
Healthcare Amenable Mortality	227	-12.0***	-.067***
		(2.9)	(.014)
Non-Amenable Mortality	91	-7.0*	-.052*
		(3.8)	(.028)
HIV-Related Mortality	17	-3.8***	-0.136***
		(0.8)	(.032)
Non-HIV Mortality	301	-15.3***	-0.042**
		(5.5)	(.019)

Notes:
* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$
Linear model reports changes in deaths per 100,000. Negative binomial generalized linear model (GLM) reports relative change in mortality. Robust standard errors, clustered at the state-level (n=50), are in parentheses. All models include year fixed effects. Linear model also includes county fixed effects, and GLM includes state fixed effects. See Equation 4 in text for full details. Sample contains adults ages 20-64.

TABLE 5: Effect of State Medicaid Expansions on Mortality, By Cause of Death, Excluding New York

Cause of Death	Pre-Expansion Mean	Linear Model	Negative Binomial GLM
All-Cause Mortality	325	-0.7	.009
		(2.7)	(.009)
Healthcare Amenable Mortality	207	-2.1	-.027**
		(1.5)	(.012)
Non-Amenable Mortality	118	1.3	-.003
		(1.9)	(.016)
HIV-Related Mortality	5	-0.1	0.009
		(0.2)	(.008)
Non-HIV Mortality	321	-0.7	0.045
		(2.6)	(.038)

Notes:
* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$
Linear model reports changes in deaths per 100,000. Negative binomial generalized linear model (GLM) reports relative change in mortality. Robust standard errors, clustered at the state-level (n=50), are in parentheses. All models include year fixed effects. Linear model also includes county fixed effects, and GLM includes state fixed effects. See Equation 4 in text for full details. Sample contains adults ages 20-64. Numbers may not sum due to rounding.

TABLE 6: DDD Estimates of State Medicaid Expansion Effect on Mortality for Non-Elderly Adults

Variable	All-Cause Mortality	Health-Care Amenable Mortality	Non-Amenable Mortality
*ExpansionState * Post-Expansion * Non-Elderly*	-.048*	-.062***	-.048
	(.027)	(.017)	(.031)
ExpansionState * Post-Expansion	-.015*	-.009	-.009
	(.008)	(.008)	(.022)
Non-Elderly * ExpansionState	.028	.050	.158*
	(.073)	(.045)	(.087)
Post-Expansion * Non-Elderly	.110***	.030***	.086***
	(.009)	(.006)	(.019)
Age 25-34	.080***	.74***	-.12***
	(.028)	(.03)	(.02)
Age 35-44	.78***	1.94***	.22***
	(.05)	(.04)	(.03)
Age 45-54	1.59***	3.05***	.59***
	(.05)	(.03)	(.03)
Age 55-64	2.41***	4.03***	.91***
	(.06)	(.03)	(.04)
Age 65-74	3.38***	4.99***	1.75***
	(.07)	(.05)	(.09)
Age 75-84	4.35***	5.91***	2.92***
	(.07)	(.05)	(.08)
Age 85 and older	5.51***	6.97***	4.35***
	(.06)	(.04)	(.09)
Male	.62***	.40***	.80***
	(.01)	(.01)	(.02)
White	.52***	.43***	.69***
	(.10)	(.08)	(.13)
Black	1.06***	1.10***	1.07***
	(.09)	(.06)	(.14)
% Hispanic	-.005***	-.002*	-.009***
	(.001)	(.001)	(.002)
Poverty Rate	.013***	.013***	.011
	(.004)	(.002)	(.007)
Median Household Income	-.000005***	-.000005***	-.000006***
	(.000001)	(.000001)	(.000001)
Unemployment Rate	.007	-.001	.013
	(.006)	(.004)	(.009)

Notes:
* p≤0.10, **p≤0.05, ***p≤0.01
Coefficients report relative change in mortality. Robust standard errors, clustered at the state-level (n=50), are in parentheses. All estimates are from a negative binomial generalized linear model, which includes year fixed effects and state fixed effects. See Equation 5 in text for full details. Sample contains adults ages 20 and older.

TABLE 7: Mortality and County Level Coverage Changes Due to Medicaid Expansions

Model and Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Years Included	All	2000, 2005-07	2000, 2007	All	2000, 2005-07	2000, 2007	All
High Uninsured Counties:	-29.4**	-	-	-	-	-	-
*ExpansionState *Post*	(12.1)						
Low Uninsured Counties:	-7.7***	-	-	-	-	-	-
*ExpansionState*Post*	(2.7)						
*ExpansionState**	-	-328.1***	-419.2***	-382.4***	-316.0***	-390.6***	-369.2***
Δ*Coverage*		(56.8)	(76.5)	(93.2)	(48.2)	(74.7)	(89.8)
Δ*Coverage*	-	100.0**	94.6**	138.0***	116.4***	99.7*	155.7***
		(37.6)	(45.1)	(39.2)	(41.4)	(54.3)	(33.2)
*ExpansionState*Post*	-	-	-	-	-23.7***	-23.8***	-18.9***
					(6.7)	(-4.9)	(5.8)

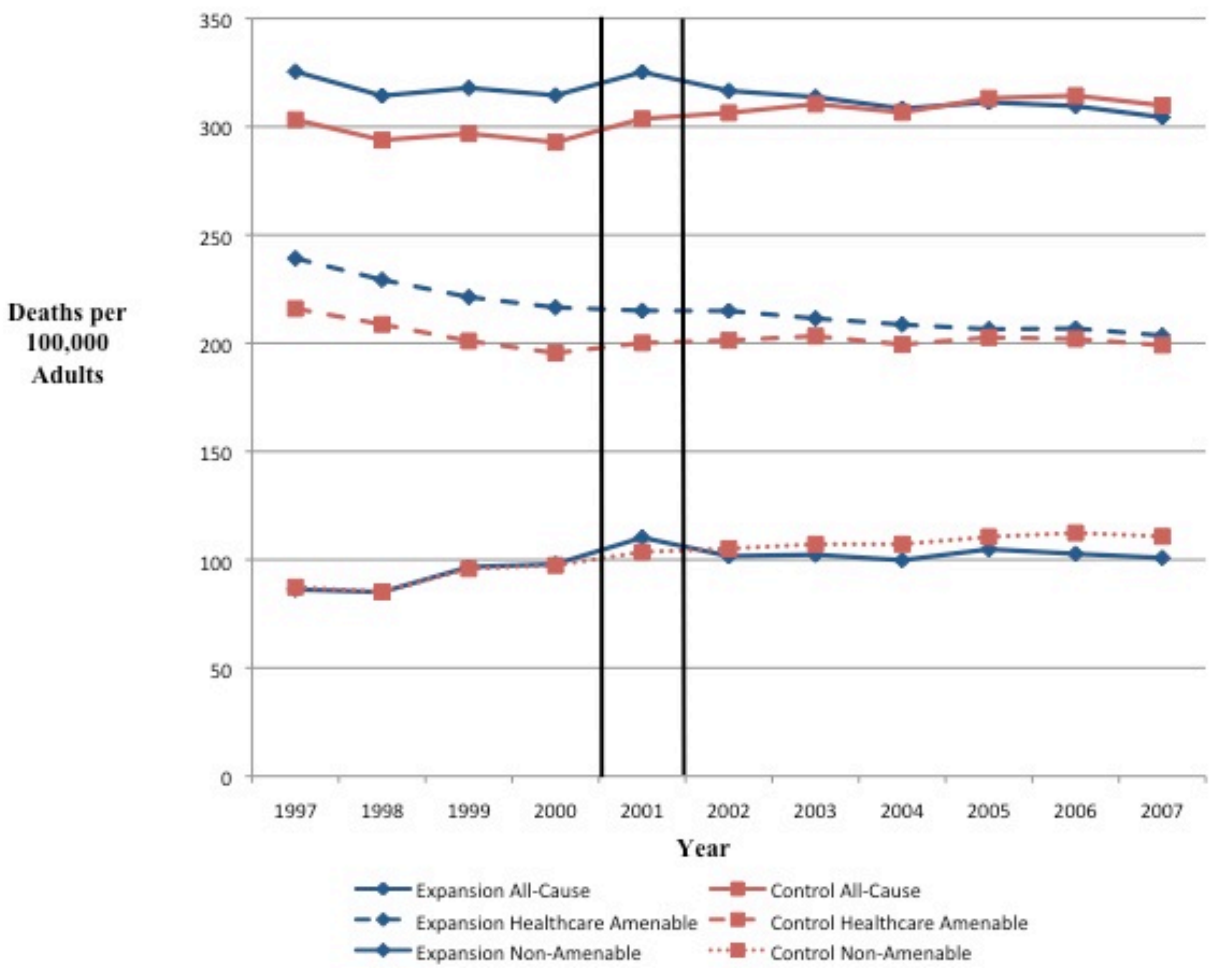
Notes:
* p≤0.10, **p≤0.05, ***p≤0.01
Model 1 is the baseline linear model with the sample stratified into high-uninsured and low-uninsured counties based on 2000 SAHIE estimates. Model 2 is described by Equation 6 in the text, and only includes the years with SAHIE estimates (2000, 2005-2007). Model 3 is analogous to Model 2, but uses only 2000 and 2007 data. Model 4 uses the full study period, with linear extrapolation used to impute the uninsured rates for the year 2002-2004, and pre-expansion uninsured rates assumed to be constant. Models 5-7 repeat Models 2-4, respectively, with the addition of the term *ExpansionState*Post.* Δ*Coverage* is the net change in the county-level uninsured rate for non-elderly adults, from pre-expansion to post-expansion. Coefficients report changes in mortality per 100,000. Robust standard errors, clustered at the state-level (n=50), are in parentheses. All models include year fixed effects and county fixed effects. Sample contains adults ages 20-64.

TABLE 8: Cost Per Life Saved Under Medicaid Expansions (2007 dollars)

		Deadweight Loss of Public Financing (DWL)		
		15%	30%	50%
Number Needed to Treat (NNT)	239	$327,000	$468,000	$656,000
	316	$432,000	$619,000	$867,000

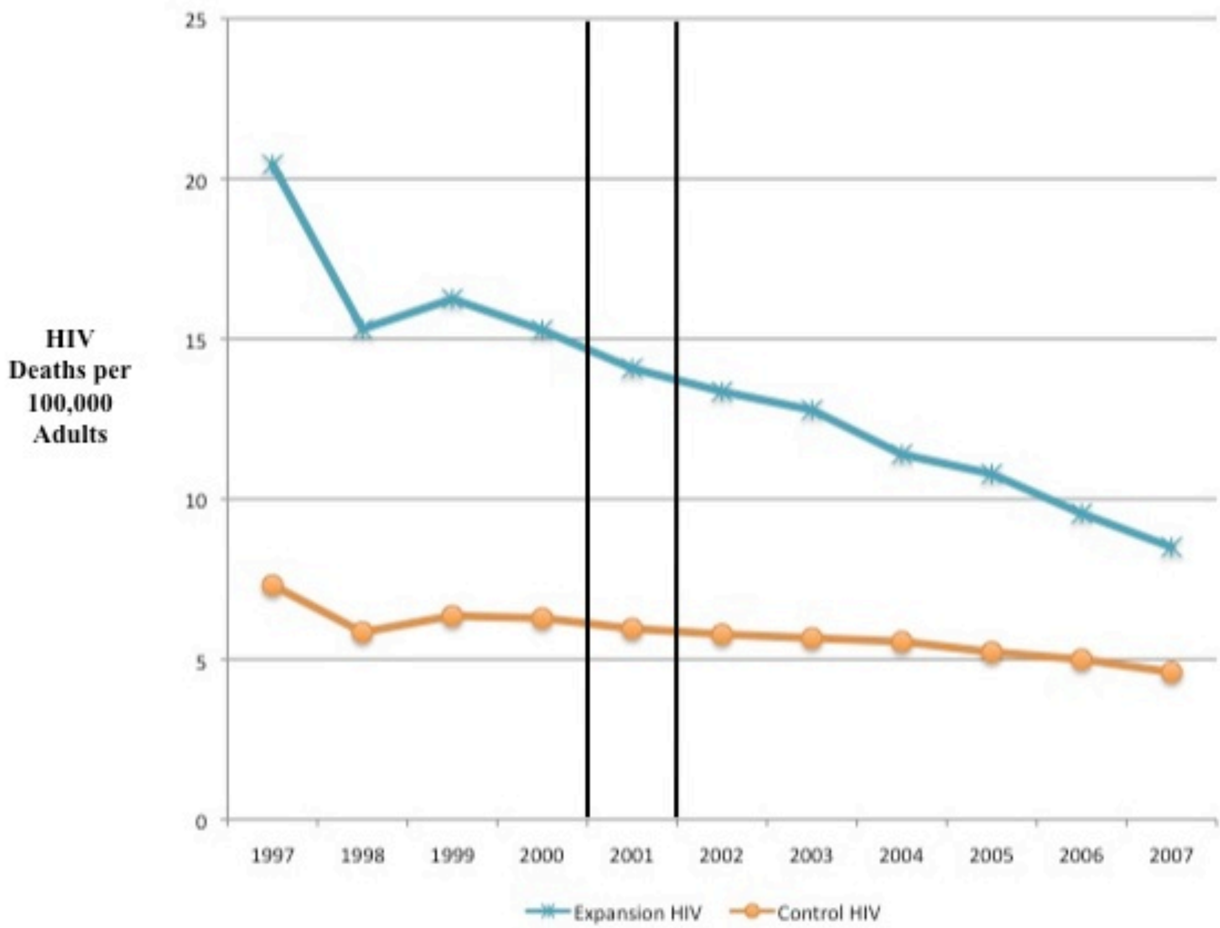
Notes:
Values come from the following formula: *Total Cost per Life Saved = NNT* * [$778 * (1+*DWL*) + $3156 * *DWL*]
Figures for additional health care spending per person with Medicaid ($778) and baseline spending for uninsured low income adult gaining Medicaid ($3156) are both from Finkelstein et al. (2012).
All terms are in 2007 dollars and rounded to the nearest thousand.

FIGURE 1: Mortality Rates (per 100,000) for Adults 20-64 in Medicaid Expansion and Control States, 1997-2007



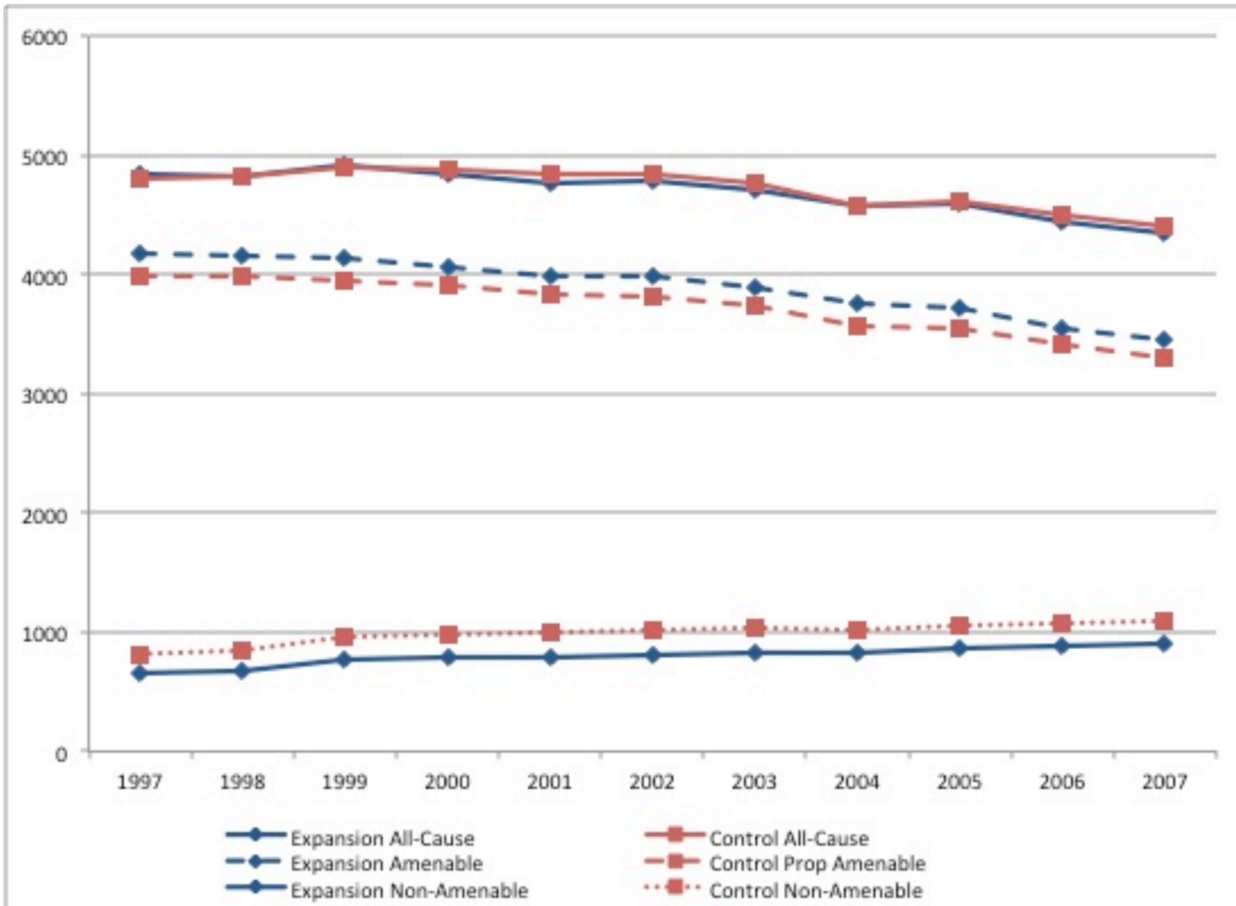
Notes:
Solid black lines indicate the beginning of the Medicaid expansions in New York and Arizona (2001) and Maine (2002).

FIGURE 2: HIV Mortality Rates (per 100,000) for Adults 20-64 in Medicaid Expansion and Control States, 1997-2007



Notes:
Solid black lines indicate the beginning of the Medicaid expansions in New York and Arizona (2001) and Maine (2002).

FIGURE 3: Mortality Rates (per 100,000) for Adults 65 and Older in Medicaid Expansion and Control States, 1997-2007



APPENDIX TABLE A.1:
Additional Robustness Checks for DD Estimates of All-Cause Mortality

Model and Variable	Linear Model	Negative Binomial GLM
Pooled Pre- vs. Post-Expansion Data:	-19.1***	-.065***
*Expansion State * Post-Expansion*	(2.5)	(.018)
State-Level Data:	-20.5***	-.059***
*Expansion State * Post-Expansion*	(4.1)	(.011)
Including 2001 as Pre-Expansion:	-18.9***	-.069**
*Expansion State * Post-Expansion*	(6.6)	(.028)
Interrupted Times Series:	-4.3***	-.014**
*Expansion State * Years Post*	(1.3)	(.006)

Notes:
* p≤0.10, **p≤0.05, ***p≤0.01
Linear model reports changes in deaths per 100,000. Negative binomial generalized linear model (GLM) reports relative change in mortality. Robust standard errors, clustered at the state-level (n=50), are in parentheses. All models include year fixed effects. Linear model also includes county fixed effects (except in the state-level model), and GLM includes state fixed effects. Sample contains adults ages 20-64.